I do not believe that we are necessitated by the far sea flight of birds, to assume a sixth sense. Is it not conceivable that birds are capable of keeping exactly the same direction of flight for many hours together, and so to fly somewhat like a shot ball or a steamship with rudder bound fast? From the physiological side, it might of course be objected that a very slight difference in the strength of the right and left wing-beats must cause a deflection from the original course, just as in the case of rowing without a steersman, a constant control by sight is necessary, if the right direction is not to be lost. To this might be replied, however, that birds are so accomplished in flight, and that we may assume they have *an extremely fine muscular sense.* Besides, they migrate mostly in company, and an error in flight of one bird will be easily corrected by the others.

But how do they hit the direction in flying away from the coast? They must be able to exactly measure the angle at which they ought to leave the land. Therein, of course, a quite small error would involve great deflections from the proper course, *but do we know that this does not actually occur often enough? and may it not be supposed that in many cases corrections are made in the flight, as soon as any point of orientation again emerges in the circle of vision? So* much we at least know, that even on land birds wander not infrequently. And it is at least not demonstrated *in any one of the cases cited by Mr. Newton,* that the birds referred to appeared on those islands *every year,* nearly *at the same time and in the same number.*

Mr. Newton adduces a second series of "facts" which seem to be against the sufficiency of the five senses; *but are these really facts?*

The young, scarcely three months old, of many of our birds, are said to pursue their flight southwards in autumn *alone.* Is that certain? and have we not here, perhaps, a too ready deduction of general rules from a few well-observed cases? Mr. Newton even says: "This seems to happen with nearly all the accipitres," &c. He quotes a letter from M. Gätke, stating that in July "Young starlings pass over Heligoland by hundreds of thousands *without a single old bird* accompanying them." I confess that I cannot regard this as a *fact,* but as a more or less probable *conjecture;* for M. Gätke, though an excellent ornithologist, could not possibly have inspected a hundredth part of these "hundreds of thousands" of starlings flying about.

I do not mean to assert that these or the other data are false; they may well be correct. I merely hold that we must guard against building far-reaching theoretical inferences on observations the general validity of which is not in the least demonstrated.

But even supposing that all these data are correct; further, supposing it certain, that these young birds, which go forth alone, also actually find the route of the species with the same certainty as if they had known it long before, would these facts be explained by the supposition of a magnetic sense? I think not. For in that case, what must have been born with the young bird? Merely this magnetic sense? *i.e.,* the power of directly perceiving external direction in its own body? By no means. There must also be born with the young bird the consciousness of what angle to the magnetic meridian it must shape its flight at.

But much more than this. It has been long known that birds, so long as they are migrating over land, frequently alter their direction; hence, supposing the young bird to be guided by a magnetic sense, there must be born with it the tendency to fly (say) twenty miles at an angle of 45° to the magnetic meridian, then 100 miles at an angle of 27°, and so on. That this is a physiological absurdity, no one would deny.

For these reasons I hold that *a special sense for direction does not exist in birds,* and that the phenomena of migration, however wonderful they appear, yet cannot ultimately depend on magic (*Zauberei*), and in this Mr. Newton no doubt agrees with me. Hence, nothing remains but to try to explain these phenomena by the known physical and mental properties of birds; *for there is no third course.*

I shall be rejoiced if Mr. Newton succeed with this better than I. AUGUST WEISMANN

Freiburg im Breisgau, March 31

THE editor having afforded me the opportunity of seeing the foregoing remarks, it will, perhaps, be convenient to the readers of NATURE that I should here add the comments I have to make upon them.

I deeply regret if my criticism of Dr. Weismann's treatise or lecture be open to the charge of unfairness. I had no wish to misrepresent him, and I cannot see that I have been guilty of such an act—indeed, the wide publication of his theory would render any attempt to do so futile. As to his acceptance of Dr. Palmén's conjecture for "absolute truths," I must urge that he took no exception to any of them, while, in the case of his Bernacle or Brent Goose, he especially adopted (p. 27) that route X which I had particular reason to consider unfounded. I did not assert that Dr. Weismann spoke of birds flying over the sea at the height of 20,000 feet, though there seems no reason why some might not, if they can do so over the land; nor did I impute to him that they always keep land in sight. I had no need to declare my disbelief in Dr. von Middendorff's magnetic hypothesis, for I never met with any man that held it. I had spoken of it already elsewhere (*Encycl. Brit.* Ed. 9, iii., p. 769), and I considered it had been set at rest for ever by Prof. Baird in the article I cited. In like manner it seemed useless to disclaim any belief in the possession by birds of a "sixth sense" which is not common to ourselves and other animals. My only object was to show that Dr. Weismann's theory was inconsistent with certain facts, and nothing he has since adduced makes me think it otherwise. As to some of these "facts" he is incredulous, and I have no fault to find with his caution in this respect, but I am sure that the more he investigates them, the less he will be inclined to demur to them. I shall leave to the ornithologists of New Zealand the defence of those that relate to their cuckoos. Dr. Weismann will find in Mr. Jones's "Naturalist in Bermuda" (London, 1859) more than enough to justify my allegations in regard to the passage of *Charadrius virginicus* (not *pluvialis*) over those islands; indeed it has long been notorious; and as to the plovers of the Sandwich group, I have not only to thank Capt. Long, R.N., for his confirmation (*suprà*, p. 460) of my statements, but also Prof. George Forbes, who kindly informs me that when there, on the occasion of the transit of Venus, he shot scores of these birds, and that his friend Capt. Cator, R.N., of H.M.S. *Scout,* having sailed thence, was overtaken in mid-ocean by them, flying in a direct line for Vancouver's Island, on arriving at which he found they had already reached it. Concerning the "facts" relating to some young birds preceding their parents in migration, the more inquiries I make of well-placed observers the more satisfactory are the answers. For want of space I cannot here give the details, but I may just say that Mr. Cordeaux, who has been for many years a watchful observer of migratory birds on the Lincolnshire coast, has named to me nine species of *Limicolæ,* of which he has personally assured himself that the young migrate apart from, and invariably arrive earlier than, the old—thus fully bearing out Temminck's assertion, made nearly forty years ago. The case of our cuckoos, which I cited, is incontestable, and M. Gätke, I doubt not, will satisfy any scruples about his starlings in that book which we are expecting from his hands.

I will also take this opportunity of replying to Mr. Pringle's note (*suprà*, p. 481). My chief reason for not referring to the matter of temperature was that we know too little of the power of birds to resist extreme cold to depend much upon it, and I thought I would not take up room by bringing in that question. Doubtless there is something in what he says touching the loom of land, but I fail to see how it will help very far, and especially in nocturnal flights. ALFRED NEWTON

Magdalene College, Cambridge, April 20

Colour in Nature

I WISH to offer a few remarks upon Mr. Wallace's kind and appreciative review of my work on the "Colour-Sense" in NATURE, vol. xix. p. 501. Mr. Wallace attributes to me "many errors" and inaccuracy as to matters of fact; but I do not think the instances he alleges are sufficient to justify the statement. Had I said in every case what Mr. Wallace makes me say, I should, doubtless, have been misrepresenting facts; but it seems to me that in most of the passages to which he refers he has slightly misconceived my meaning. I should not attempt to oppose so distinguished a naturalist on points of biological inference, but I venture to defend the accuracy of my statements of fact.

1. "*Scissirostrum Pagei* does not 'belong to a family generally dull,' while it is itself decidedly dull-coloured." The first statement will be correct if we place *Scissirostrum* among the brilliant starlings; but Mr. Wallace himself, following Prince

Lucien Bonaparte, puts it next to the West African *Buphagæ*. Now the *Buphagæ* are certainly dull birds, while *Scissirostrum* is described in the "Malay Archipelago" as "almost entirely of a slaty colour, with yellow bill and feet, but the feathers of the rump and upper tail-coverts each terminate in a rigid glossy pencil or tuft of a vivid crimson" (i. 430). I wrote with this passage of Mr. Wallace's under my eyes, and refer in a footnote to his volume for the vivid crimson. I did not say the bird was brilliant, I merely noticed the colour of its tail and beak. The case really stands thus: If *Scissirostrum* was differentiated from a generic ancestor generally resembling *Buphaga*, we have to inquire, why did it develop these ornamental adjuncts? and my answer is, because while *Buphaga* pecks the parasites of the backs of mammals, *Scissirostrum* feeds off "grains and fruits."

2. "Santarem, of which it is said 'the pastures are destitute of flowers, and also of animal life, with the exception of a few small plain-coloured birds,' is one of the richest localities for flowering shrubs in South America." Now, this passage to which Mr. Wallace takes exception is not mine, but is a textual quotation from Mr. Bates ("Naturalist on the Amazons," p. 183). It is given in inverted commas in my text, with reference to the original in a footnote. I was, of course, aware that the Brazilian woods generally were full of brilliant birds, and that "the butterflies in the adjacent forests were gorgeous in the extreme." What I wished to point out was that in particular spots like these meadows, where the general aspect of the flora was not bright, the purely local fauna was likewise dull. We may find great varieties in this respect nearer home in a meadow, an adjacent warren, and a moor or swamp behind it. Moreover, the passage was professedly quoted, simply as showing the general impression left upon my mind by reading various books of travel. May I add a sentence from a private letter of Mr. Darwin's, which helps out the same view on a larger scale? "The contrast," he says, "in the colour of the birds in Patagonia" (where he had just noticed "the sombre aspect of nature"), "and on the bright green flower-decked plains of La Plata is very striking."

3. About a certain squirrel, described in the "Malay Archipelago" as having a tail "ringed with gray, yellow, and brown," and as looking "exceedingly pretty," Mr. Wallace now says it "is one of the dullest of the group," while he did not "say a word about its feeding on 'bright-coloured fruits.'" But he *did* say that it would eat "any fruit" (i. 192), and I presume, therefore, that it sometimes eats "bright-coloured food."

4. "So far from the colours of caterpillars being 'mostly protective,' every entomologist knows that a large number of caterpillars in every part of the world are conspicuously coloured." True; but Mr. Wallace himself was the first to suggest that these conspicuous colours were themselves protective by giving warning of inedibility; and I am at a loss to understand what he means by thus going back upon his own words. I took my statement from Sir John Lubbock's lecture "On Certain Relations between Plants and Insects," pp. 23–24, where this fact of universal protective colouring in larvæ is very clearly brought out.

5. "Again, the ground-feeding pheasant family are passed over as containing only one brilliant bird, the peacock, whereas it abounds in species of the most gorgeous colour." But my words are very different from this—"Even among the pheasants themselves," I say on p. 176, "many species are far from brilliant; and when we come to compare the whole family with that of the parrots or the humming-birds, we shall find that the peacock alone can fairly come into competition with the typical fruit-eaters and flower-feeders." Mr. Wallace goes on to mention (amongst others) the "Impeyan pheasant of the Himalayas," and "the intensely-brilliant fire-backed pheasants of the Malay countries," as among the most brightly-coloured species. Any one would suppose from his review that I had totally overlooked these cases; but in the very same paragraph with the sentence which Mr. Wallace blames the following passage occurs:—"The forests of the Himalayas and the Malay Archipelago, with their great brilliant fruits and flowers, and their exquisite insects, form the haunts of the most beautiful species of pheasants" (p. 177). As a matter of fact, before writing that paragraph I had carefully compared all the living *phasianidæ* in the Zoological Gardens, and all the preserved specimens in the British and Oxford Muesums; and I feel sure that any one who does the same will agree with me that the peacock alone can be placed in the very first rank of brilliant colouration.

6. How much the subjective element enters into these questions may be seen from the following remark of Mr. Wallace:—"The tigers, the zebras, the beautifully-marked antelopes, and the spotted deer and giraffes, *which are really among the most brightly-coloured of all mammals*, are passed over as less beautifully coloured than the squirrels and monkeys." Now I confess myself simply astounded at the statement that the zebra, of all animals in the world, is brightly coloured—a creature without a tinge of anything but creamy white and black about its body. Quite apart from the nature of food or surroundings, I call a panda a brightly-coloured mammal; or a mandrill; or a Rhesus monkey; or a Canadian chipmonk; but certainly not a tiger, a zebra, or a giraffe, none of which has a single tinge of scarlet, blue, green, or bright yellow.

No one who knows anything of Mr. Wallace could for one moment imagine him capable of intentionally misrepresenting the humblest opponent in the smallest particular; and I owe him many thanks for much kind and appreciative criticism both on this and several previous occasions. Yet I cannot help thinking that in these instances, and others with which I will not burden your space, he has unconsciously permitted mere differences of opinion unduly to assume the appearance of positive errors in fact.

GRANT ALLEN

Remarks by the Reviewer

1. *Scissirostrum Pagei* is universally placed in the starling family. Its affinity to *Buphaga* is very doubtful, while its *crimson-tipped tail-coverts* are very different from "*a tail of vivid crimson*" which Mr. Allen gives it (p. 184).

2. I object altogether to founding theories on chance expressions of travellers. It is curious, that in my "Travels on the Amazon" (p. 157) I refer to these same Santarem pastures as follows:—"There were some boggy meadows here, more like those of Europe than one often sees so near the equator, on which were growing pretty, small *Melastomas* and other flowers. The paths and campos were covered with flowering myrtles, tall *Melastomas*, and numbers of passion-flowers, convolvuluses, and bignonias." These open meadows and campos really exhibited more conspicuous flowers than the woods and forests which swarmed with brilliant butterflies and birds.

3. I referred to the squirrel, because it was the only example given by Mr. Allen which I could at the moment test.

4. My argument is, that the colours of caterpillars are often as varied, as vivid, and as beautifully arranged as in birds and winged insects. This is not necessary for protection by *conspicuousness*, for which purpose any tint contrasted with foliage, such as black, or white, or ringed with black-and-white, would have sufficed.

5. The "pheasant" question I leave, as Mr. Allen has placed it, for the consideration of naturalists.

6. Here it seems to me Mr. Allen is himself changing his ground. His main argument is that the æsthetic tastes of the higher animals are the same as ours, yet he objects to the elegantly-marked and intensely-contrasted zebra and tiger being called "brightly-coloured." Surely they are more beautiful than the mandrill or the Rhesus; while among animals *white* is as much a colour as among flowers.

ALFRED R. WALLACE

Nitric Acid Batteries

I INCLOSE the results of some experiments I have lately made to ascertain if the cost of working the nitric acid batteries of Grove and Bunsen could be reduced. I find that the nitric acid can be replaced by a mixture of half nitric and half dilute sulphuric. And the latter gives a higher force for nearly three hours. The experiments were made with a large-surface voltameter, and the gases were collected during one minute every half-hour; four pint-size cells were used. The experiments were repeated, and every care taken to avoid any error. I have also used the mixed acids very successfully with twenty-eight cells for the electric light. I presume the increased power is due to the internal resistance of the battery being slightly lowered by the addition of the dilute sulphuric acid in the porous cell. I may add that the fumes were much less than when nitric acid alone is used.

JOHN HENRY KNIGHT

Farnham, April 19

The Black Rat

IN regard to the distribubution of the black rat (*Mus rattus*), your correspondent may be glad to know that this animal, spread